stated. In fact, Prof. Weldon's original interpretation has not yet been, and never may, be disproved, and, for all we know to the contrary, selection may still be acting in the direction indicated by his measurements. Mr. Bather will see, on reference to my letter, that I advanced the idea "for whatever that idea may be worth." It appears necessary to point out that the "facts" are before us in this case; it is a question of interpretation. The difficulty raised by your correspondent is one of his own creation, and not one of mine. Where have I stated, or even suggested, that the appearance of a character which formerly was of selection value, and which now appears in the later stage of individual development, is a "universal principle"? If the suggestion which I made with reference to the interpretation of Prof. Weldon's results is sound in principle, it is obvious that it refers to a (comparatively) recent cessation of selection, and so far Mr. Bather has correctly grasped my meaning. When, however, he lays it down, on his own responsibility, that the conditions then "can hardly have been very different to now," he is laying claim to a knowledge of the past and present conditions of life and to a familiarity with the time necessary to bring about the modification of species, at which I can only stand aside with envy and admiration. If Mr. Bather really must have some "known fact" after this distinct dissociation of myself from his methods, I will refer him to those cases of mimetic butterflies which have no models. There is good reason for believing that certain butterflies which are undoubted mimics, since they depart from their allies in type of pattern, have for some unknown reason survived, while the species which they imitated have, also for unknown reasons, become extinct. The mimics still retain their mimetic pattern and colouring, but the selective process which produced this type can no longer (in the absence of the model) be regarded as in active operation. Nevertheless, the mimetic disguise is retained by virtue of heredity, and appears in the last stage of the ontogeny. Your correspondent will, I hope, pardon any apparent discourtesy if I state that the correspondence on this subject is, so far as I am concerned, now closed. I can assure him that he shall hear more about the results of Prof. Weldon's measurements at no very distant future. R. MELDOLA.

Measurements of Crabs.

MR. CUNNINGHAM, in NATURE of October 29, raises a doubt as to the trustworthiness of the results of my measurements of crabs. on the ground that the specimens of the year 1893 had been longer in spirit than those of 1895.

The value of measurements made on animals preserved in spirit naturally depends on the animal so preserved; and to any one who is acquainted with the rigid nature of the calcareous carapace of *Carcinus mænas*, it seems difficult to admit the possibility of distortion from such a cause. All specimens that were not quite rigid (a condition due to the animal having moulted just before capture) were rejected as a matter of course.

The fact that a deficiency in one dimension was "compensated" by an excess in the other dimension, so far from suggesting to Mr. Cunningham a suspicion of the specimens having "undergone an artificial change of shape," would, I think, have seemed not only natural, but a feature to be expected, had he realised that every crab during its growth passes through a similar change of shape, in which the relative increase in size of the one dimension (the dentary margin) is accompanied by a corresponding relative diminution in size of the other dimension (frontal breadth). The table appended to the paper in question shows that, while the dentary margin in the youngest crabs measured increased on an average from 414 thousandths of the carapace length to 498 thousandths in the adult, the frontal breadth, on the other hand, diminished in relative size from 813 thousandths in the young to 595 thousandths in the adult; so that a compensatory relation between these two parts of the hard skeleton is a normal phenomenon in these animals.

I would venture to add that observations in recent years, on the variability of both animals and plants, tend to show that species are much more unstable than was once supposed. The changes recorded in these crabs in the space of two years are very minute, but they are persistently in one and the same direction in all the twenty-six stages of growth measured. If such a rate of change exceed what we should have expected, it may be because hitherto we have had no collections of comparative facts on which to base any reasonable expectation. H. THOMPSON.

October 30.

I AM able to offer Mr. Cunningham direct experimental evidence that the hard carapace of a young shore-crab is not sensibly distorted by immersion in spirit for six months.

I have lately had occasion to compare the mean frontal breadth in three samples of young female crabs, gathered from the same locality in Plymouth Sound. The range of size was the same in all the samples. The samples were (1) a large number of individuals, collected in 1892 and 1893, measured after immersion in spirit during a period varying between six months and a year: the mean frontal breadth in these crabs was compared (2) with that of 569 individuals, collected during the summer of 1895, and measured fresh while still wet with seawater; and (3) with that of 595 crabs, gathered also during the summer of 1895, but preserved in spirit for about six months before measurement.

The mean difference between the frontal breadth in the spirit specimens of 1892-3 and that of the fresh specimens of 1895, was 1·47276 of Mr. Thompson's units; the difference between the crabs of 1892-3 and the spirit specimens of 1895 was 1·58992 units. The difference between the two results obtained from crabs of 1895 was therefore only 0·11716 unit—a difference so small, compared with those observed by Mr. Thompson, that it may safely be neglected, even if it be assumed to be due entirely to the spirit, and not to be within the probable error of the determination. As for the distribution of deviations from the mean in the two samples, it would take too much space to compare them in detail here; I hope to do so before very long in connection with another problem, and therefore I will only now say that the "standard deviation" (= error of mean square) of the series of fresh shells was in Mr. Thompson's units 11·7955, that of the spirit specimens being 11·9628 of the same units.

These figures seem to me sufficient to justify Mr. Thompson's conclusion; and it must therefore be held that a change, whether of oscillation or of evolution, is going on at a measurable rate among *Carcinus mænas* in Plymouth Sound.

The very great importance of this result to all students of animal evolution is evident; for the careful study of cases in which change is actually going on so rapidly that it can be watched and measured in a reasonably short space of time will assuredly be found to give the best, if not the only clue to the process of evolution in general. W. F. R. WELDON.

University College, London, October 31.

The X-Rays produced by a Wimshurst Machine.

IN my two papers of June 4 and June 18 respectively, I proved the existence of non-homogeneity in the X-rays, and gave a simple method of strengthening and maintaining the discharge of these rays from an ordinary focus tube by means of a conductor wrapped round the part of the tube level with and behind the kathode, and separated by a small sparking gap from either an earthed wire or the kathode's external wire loop, or the wire leading from the induction coil to the kathode itself. Since then my attention has been chiefly turned to the most remarkable results which can be attained with a Wimshurst machine, with which this paper is concerned. My Wimshurst has two 15-inch plates, and, not being of the latest type, has the old-fashioned metallic sectors and buttons, instead of the plain varnished glass disc, though I do not imagine that the results will be found materially different with the simpler, and probably better, form.

At first I drove the machine by hand, and hand driving is sufficient for the results mentioned in this paper—a most important fact, seeing that it renders unnecessary any other form of engine than the hand, and brings the copious production of any kind of X-rays within the reach of those who have neither battery nor dynamo—afterwards, for securing greater personal freedom and uniformity of turning force, I drove the Wimshurst by a small motor, an easy and most convenient plan, using a platinum electrode acidulated water resistance by which the rate of rotation is easily governed.

To me, accustomed only to the effects given by a 4½" spark induction coil, the brilliancy of the shadows given by simply connecting my tube's electrodes to the brass knob terminals of the Wimshurst used without condensers was surprising, and the steadiness of the image a most grateful rest to the eyes. The bones seemed more transparent than I had ever seen them, and though at first I thought this might be only the effect of the lessening contrast brought about by increased general illumination, this does not prove to be the case. The two faults to be found with the Wimshurst used in this way are, first, that after a rest or complete discharge the polarity of the machine is apt to reverse, the cure being either (*a*) to completely discharge again and trust that the desired reversal will take place on re-starting, a very uncertain plan; (*b*) to interchange the wires, probably best by disconnecting them at the tube and turning the tube suitably; or (*c*) by discharging completely and starting the electrical action of the machine by holding in some position, found easily by experiment, a body electrified and whose electrification is of known sign—*e.g.* a glass rod rubbed quickly and lightly on a piece of silk or flannel. The second fault is that in this way of using the Wimshurst, whether the condensers are in circuit or not, after a time the character of the discharge is apt to alter. Beautiful patches and streaks of very bright grass-green make their appearance on the inner surface of the glass; and when one of these has formed, the X-ray discharge becomes weaker, and two or three are altogether fatal to it, even if the machine be in excellent working order. When once a patch is formed, too, it seems to cling obstinately to the place where it develops. It was suggested to me that these patches were invariably associated with bubbles in the glass of the tube, but on touching the tube lightly with a camel's-hair brush dipped in rather dry Indian ink, and thus painting over these green spots whilst they were visible, and afterwards removing the ink spot by spot and examining for bubbles, I found in many cases they were not present, and also that there were many bubbles where no fluorescent spot had been painted. These spots lie in the shortest lines which can be drawn on the glass from the kathode to the anode. They are far more numerous near the kathode, forming near it, and often gradually elongating towards the anode. Blowing gently on the middle part of the tube causes their retreat towards their birthplace, and often extinguishes them altogether. Their forms seem to be those which slightly divergent jets of matter would take if they were thrown out from different points on the sharp edges of the kathode and impinged on the inner surface of the bulb very near the kathode, exciting fluorescence so long as the matter maintained a velocity above a certain limit. If this is so, and the matter is negatively charged, and if the glass surface acts to it as a rough surface does to a stone, perhaps on account of its high electrical resistance, it is easy to see why the patches are small and slowly spread when a higher E.M.F. is used, and how it is that the dissipation of the positive charge which lies on the outside of the bulb leaves these jets of negatively electrified particles more free to take a shorter path to the anode. Thus, if they hit the glass at all, they will do so further and further from the kathode; and this explains why the fluorescent patches travel towards the anode region of the bulb, when the neighbourhood of the kathode is deprived of its positive charge by gently breathing on the glass in this part of the bulb. Similarly it also explains why the patches travel kathode-wards when the middle and anodal region is drained in any way of its positive charge, for then the attraction in the kathodal region is practically strengthened by the destruction of the attraction to other glass parts of the tube. Not only do these jets impinge on the inner side of the glass just by the kathode under ordinary circumstances, when no pains are taken to drain the external positive charge of the tube, but they are partly reflected, and, owing to the attraction of the positive charge, the path of the reflected particles is bent towards the glass surface, and in many cases a fainter patch of longer shape is formed further from the kathode, and I believe a third may be, so that the inner surface of the tube tends to become filled with patches smallest, brightest, nearest together and least elliptical, near the kathode; and becoming progressively larger, fainter, further apart, and longer in proportion the nearer they approach the anode. This has a very important application, for seeing that the formation of the patches is, by experiment, inimical to the creation of the X-rays, anything which tends to their formation must be avoided; and if, as I am convinced, they are due to the sharp edges and small irregularities of the circular kathode, no pains should be spared to give that electrode a perfect polish on the side facing the anode, and to make its edge circular, and not square, as the circular kathode's edges are in my tubes. Another way of partly overcoming this difficulty is to make the tube very wide round the kathode, or, which amounts to the same thing, push the kathode well forward into the globular part of the tube. This last is done in some tubes, I believe, with excellent results. A striking experiment is to arrange a tube and Wimshurst to give the green patches, and then to breathe rather strongly on the kathode half of the tube two or three times if necessary, and watch how the phosphorescent patches fly towards the anode as the external positive charge is dissipated and, at last, just meet behind the anode (which region becomes brightly fluorescent); finally, if the positive charge is sufficiently dissipated, part creeping and part bounding along the glass tube which, in my tube, encloses the anode's wire support, into which support, or the actual back of the anode, they finally yield their negative charge. I think that any one, who will take the trouble to make the very easy experiments here recorded for himself, will feel convinced that this is the true explanation of many of the—so far as I know—widely observed, but unexplained variations in the behaviour of the X-ray tubes, and the last experiment suggests that it would be a good thing to (i.) completely surround the anode stem and the back of the anode itself with some insulator—glass would probably prove best—and if the anode be platinum, to make the anode's edges very blunt and smooth; and it may be well, but of this I cannot feel certain without experiment, to make a small area, large enough to include the point from which the X-rays seem to emanate, rough with, say, platinum black, in order that over this area, to put it in old-fashioned language, the density of the positive electrical charge may be as great as possible.

In these ways, perhaps, the stray negative "jets" may be prevented, and a tube made to emit the X-rays more easily and steadily than any at present used.

It seems natural, from these conclusions, to suppose that pushing both anode and kathode far into the tube so that they are fairly close together, is a very good plan, for it would certainly tend to prevent leakage to the internal surface of the glass. I have heard that very excellent results have been obtained with tubes in which the electrodes are but a very few millimetres apart. The same reasoning would indicate that it would be well to make the kathode convex towards the anode, and fairly small, but not very small; for each tube there will be a special size which will be best.

Whatever the nature of the "jets" may be—and I suppose most will be inclined to believe (with Crookes) that they are particles of the residual air, for there is very little evidence of any scattering of an aluminium kathode, save just round its edge—it is clear that the effect of the external positive charge is to create a higher vacuum in the central portion by drawing the particles to the sides of the tube; and this accounts for the action of a flame on the glass bulb, which is two-fold—for it not only drives off from the sides, and possibly from the electrodes, particles of moisture or occluded gases held by some force other than electrical, but also speedily dissipates the external charge, and thus frees the "electrically bound" molecules, distributing the matter more evenly over the internal space, and thus making the passage of electricity between the electrodes easier. At the same time the phosphorescence, which is produced by their impact on the glass, becomes evenly distributed over the tube, and it is whilst hot that the X_2-rays are emitted (to which wood is transparent, but flesh opaque); and it should be particularly noted that these X_2-rays are in this case emitted when the tube offers less resistance to the electrical discharge than when the X_1-rays (which penetrate flesh, but to which bone is fairly opaque) are being emitted.

Several observers—in particular, Mr. Swinton—have mentioned that under some conditions a tube is capable of emitting rays to which bone is almost as transparent as flesh. These I shall call throughout the rest of this paper X_3, and in the experiment I am about to describe it will be found that it is possible—and, indeed, very easy—to cause a tube to emit either X_2, X_1, or X_3-rays; and not only so, but inasmuch as the change from X_2 through X_1 to X_3 is perfectly continuous, it is simplest to believe they differ, not in kind, but only in some one inherent quality, such as frequency, which varies continuously. I may say, beforehand, that the following experiment seems to me a very important one, and the study of it likely to lead to some definite conclusions as to the undulatory nature, and even evaluation of the wave-lengths of the X-rays.

Two small Leyden jars then are attached to the Wimshurst in the ordinary way; two well-coiled insulated wires, which are supported on insulating posts, terminate in brass knobs at the ends next the prime conductors of the Wimshurst, and in carefully-made small smooth loops of bare wire at the four ends. There are thus four spark gaps (and it is impossible to overrate the importance of the adjustment of these spark gaps in using the Wimshurst for this, or any other experiments where fluorescent screen or photographic effects are required—why, will appear later). For brevity I designate the spark gaps thus:—

(N) Gap between negative knob terminal of Wimshurst and coiled wire to kathode.
(K) Gap between coiled wire to kathode and the kathode external wire loop.
(P) Gap between positive knob terminal of Wimshurst and coiled wire to anode.
(A) Gap between coiled wire to anode and the anode's external wire loop.

Experiment i.—Make N and P about ½″, A exceedingly small, and K about ¼″. The result, when the machine is being turned by hand as quickly as is convenient, is a series of discharges, during each of which the tube flashes out a brilliant, almost orange-green, giving on my fluorescent screen a light so bright as to be trying to the eyes; the hand will show at once that X_3-rays are being emitted, and with this arrangement I have shown the ordinary shadow experiments to a large room-full at once. Three or four persons can see at the same time the back-bone and ribs of a man if a little care is taken to exclude extraneous light; and what I expect will interest a great many people who are not scientific, I have seen (to put it popularly, though of course incorrectly) through a brick wall, *i.e.* through 8½ inches of solid brick. To speak exactly—the rays which come through the brick are sufficiently powerful to show dullish flashes on the screen, and a piece of platinum foil placed just behind the screen is distinctly visible, though badly defined, during the flashes. It is necessary, of course, to be careful to avoid any ordinary light reaching the screen, or any X-rays from reaching it except through the wall; but the experiment has been performed carefully many times now, and there is no doubt whatever as to the power of the rays from even my small tubes to penetrate this thickness of brick. These X_3-rays have wonderful penetrating power, and experiments with them are well worth making. The X-light is able to penetrate a little over 1½″ of glass, and 3″ of water easily, and 37″ of wood.

In this experiment, unless the spark gaps are adjusted carefully (and the measurements I give are only intended as a rough guide), sparks pass along the outside of the tube in a way which at first made me anxious for the tube's life; but so long as the tube's loops are nearer to the coiled wires than any other part of the tube, experience teaches me there is no danger. Even during the passage of these external and noisy sparks, there is a discharge inside the tube sufficient to show a faint and very transparent hand shadow on the screen, the bones being scarcely distinguishable from the flesh—these being the most transparent shadows I have yet seen.

Experiment ii.—Arrange so that A is exceedingly small, and N and P about equal, and so that the discharge by long sparks along the tube and outside it are just avoided; then take a piece of thoroughly wet string, and fasten one end of it to the kathode loop, and the other to the end of the coiled kathode wire (a few strands of lamp-wick answer even better), and move the kathode coiled wire away from the tube's neighbourhood, carrying (of course) one end of the string with it until there is no direct sparking through the air, and all the discharge goes through the string. If the radiation from the tube be now examined by the screen with the hand held close behind it, the shadow of the flesh will be very dark indeed, and the bones scarcely visible; in fact, it is fairly easy to secure a quite black shadow whilst the rest of the screen is brightly illuminated. These are, therefore, the X_2-rays. By shortening the string gradually the shadow changes, the flesh becoming more and more transparent, the bones' shadows remaining black for some time, until when the direct disruptive discharge through the air begins the X_3-rays are immediately restored, and the radiation penetrates easily both flesh and bone. Thus, by "loading" the circuit more or less, the character of the radiation is altered. We know that the discharge of a Leyden jar is oscillatory, and the frequency of vibration very high, also that the frequency is lowered and the oscillations damped by the use of a wet string or other resistance placed in the jar's discharge circuit; hence it appears reasonable to suppose that it is this slackening of the electrical oscillations which produces the corresponding change in the X-rays, and that this slackening can be produced by altering resistance outside as well as inside the tube, at any point, perhaps, in the circuit, so long as the discharge is kept disruptive in character. In this experiment the discharge through the three spark gaps, and so through the whole circuit, is certainly disruptive; but the influence of the wet string on the whole circuit, including, of course, these gaps, is shown strikingly by the altered appearance and sound of the sparks at N and P. The wet string, therefore, causes the tube to produce rays of the same kind as the heating of the tube, and if heat increases the general conductivity, it would seem that the wet string must do the same thing. It is, besides, obvious that the wet string takes the place of a rather wide air gap, and it can be very easily proved by experiment that the air gap possesses the greater resistance. Hence it might be thought that the vibrations in the case of air would be more slowly executed; but in these experiments we have to do, not with the ordinary resistances of air and wet string, but with their resistances during the disruptive discharge; to put it into popular language, the air can stand a great electrical stress without giving way, but when once its initial and great resistance is overcome, its resistance may be for the time very greatly diminished, so that the electricity surges backwards and forwards, it may be, many millions of times in a second; whereas in the case of the wet string, although it opposes at first far less than the air, yet its resistance never breaks down completely, for this very reason. The air and the string may be compared in this respect to an oak and a reed in a gale of wind; the reed, though it bends still resists, is always resisting, and the more it is bent the more it resists; the oak stands unmoved till it is broken or uprooted, and its resistance overcome. That the oscillatory discharge is necessary for the production of the X-rays I feel no doubt, though it would be an extremely interesting experiment for any one, who had the means and leisure, to try whether they could be produced by a so-called continuous current from a battery. A very few thousands of cells would seem likely to be necessary; but if any one should construct a tube according to the hints and instructions given in this letter, I think that the number which would be found necessary might be a good deal less. The discharge of a Wimshurst without Leydens, and with the four spark gaps nil, is still disruptive, though I expect less so when (unlike mine) the plates have no metal buttons and sectors on them; the rotating mirror, I believe, shows that even the silent brush discharge is disruptive—certainly the thin blue sparks are so, and the discharges from the revolving plates on to the combs and between the two plates themselves, are all disruptive.

Experiment iii.—The spark gaps being arranged as in Experiment ii., instead of the wet string the secondary circuit of my 4½″ spark induction coil was inserted, the rays given off by the tube were then X_1-rays, which gave most exquisitely bright and clear pictures on the screen, whilst at the same time the discharge through the coil gave rise to very decided Hertz effects for a considerable distance. It is clear that a rheostat of some fine wire of high resistance would be convenient to use with a tube, and that by its means we could adjust the kind of X-ray evolved very nicely.

The nature of the spark discharges at N and P during the emission of the X-rays deserves close attention. At times the spark seems like a string of equidistant silver beads strung on a bluish violet thread, and suggests stationary waves. Perhaps I may add that my work is necessarily interrupted for some weeks at least. I hope that any one who finds in this letter suggestions he would like to follow up experimentally, will not fail to carry out his wishes. The form of kathode I should recommend is a concave mirror focused on the anode, the outer rim being bent back so that the edge is well concealed behind the mirror, a section through the centre of the mirror face having a form something like a very shallow sign of Aries.

Eton College, July 24. T. C. PORTER.

Extension of the Visible Spectrum.

REFERRING to the interesting letter on the above subject, from Prof. Oliver Lodge and Mr. B. Davies, in your issue for the 29th ult., I should like to mention that I have observed a similar extension of the visible spectrum when thrown upon a fluorescent screen of barium platino-cyanide. I may add that a screen of this description becomes brilliantly luminescent when brought into the vicinity of the brush discharge from a large Wimshurst machine, or, better still, from a Tesla coil. In the latter case the fluorescent surface will become luminescent at a considerable distance from the electrical discharge, if facing the latter; but if the screen is held with its opaque backing towards the discharge, the platino-cyanide will only luminesce if actually penetrated by the streamers of the discharge.

This screen will also fluoresce brightly at several yards distance from an ordinary Geissler vacuum tube.